

# MONTHLY NOTICES

## OF THE

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EDWIN DUNKIN, F.R.S., President, in the Chair.

Samuel Palmer Chapman, 27 West Park, Clifton, Bristol ;  
Duncan Forbes, 11 Hanover Buildings, Southampton ; and  
Charles Horsley, F.G.S., 174 Highbury New Park, N. ;

were balloted for and duly elected Fellows of the Society.

*Notes on Nyrén's Determination of the Constant of Aberration.*  
By David Gill, LL.D., F.R.S.

(Extract from a letter to Professor Otto von Struve.)

Nyrén's remarks on the possible systematic errors that may attach to determinations of the constant of aberration bring strongly to my recollection the course of thought which led me to write the article which you will find at pp. 40-43, vol. i. of "The Observatory."

I there mentioned that W. Struve's determinations of the constant of aberration exhibited indications of a term depending on the R.A. of the star observed. Nyrén's determinations also exhibit variations which are still more pronounced, and which can be very accurately expressed by terms depending on  $\sin a$  and  $\cos a$ —that is by  $\sin (M + a)$ ; but the value of  $M$  would be quite different if it were deduced from his and from Struve's series.

Struve's series depends on too small a number of stars (notwithstanding the great accuracy of each result) to enable us to arrive at very accurate ideas as to the value of  $M$ ; still, the fact that  $M$  is very different if deduced from Nyrén's series points to the probability that Struve's results (if Nyrén's are accurate) were affected by systematic changes in the clock rate or azimuth, which were more or less eliminated by the methods employed by Nyrén. I quite agree that the precautions adopted by Nyrén of comparing his clock before and after each set of observations with the normal clock (situated in air of uniform pressure, and

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where the diurnal variation of temperature is insensible) enable us to dismiss from further consideration any errors on account of the clock.

But can we entirely dismiss errors depending on the azimuth? If I remember rightly, the marks employed and their lenses were not sheltered from the Sun quite in the same way as the azimuth mark of the Transit instrument; at least we have no detailed account of their construction.

Also, since azimuth marks in the prime vertical and the ground on which they are situated are exposed to greater causes of disturbance in azimuth than similar marks situated in the meridian, it is of the utmost importance that all care should be taken to obtain evidence of their freedom from diurnal variation.

[To explain further what I mean, the Pulkowa meridian marks and the soil on which they stand are exposed chiefly to the Sun from the south, which would tend to create their chief variations in the direction N. and S. (I speak of the diurnal effect as a whole), which, therefore, might tend to change their azimuth more in a N. and S. direction than E. and W.]

Now, as you have two and not four marks, you cannot from the marks alone separate the change of azimuth from change of collimation. But it would be of great interest to compare *separately* the azimuths of the two marks, employing the value of the collimation derived from the star observations, which collimation might be assumed to have a constant relation to the value of the collimation when the instrument is horizontal (of course you could not assume the collimation identical for the vertical and horizontal positions of the telescope). If by this means it could be shown that the relative azimuths of the marks show no diurnal variation of mutual azimuth, the determination as a whole, it seems to me, would have even a higher value—high as the present value, in my eyes, is. Of course I admit that the effect of any such systematic error is nearly (but not quite) eliminated by the distribution of the stars in R.A.; and I agree with Nyrén in the opinion that the definitive result of his researches is, on this account, more accurately estimated by giving equal weight to the results derived from stars in each quadrant of R.A., than by giving each separate result a weight corresponding to its probable error.

But if M has a real existence, it is a fact of the highest cosmical interest, and I think that no trouble should be spared to determine it with the highest possible accuracy.

If M were accurately determined at Pulkowa, and another value  $M^1$  as accurately determined here, we should have from the two mean values of aberration and the values of M and  $M^1$  the necessary data for determining whether or not the galaxy has a motion of translation in space, at least within certain limits; and I see no reason to suppose it impossible that that translation may amount to thirty or forty times that of the Sun's

translation relative to neighbouring stars—at least, I think Nyrén has dismissed the matter somewhat too absolutely. There is at least in his results very strong evidence of the existence of *M*, and this should be traced to its source.

With this in view I venture to suggest the following points beyond a further examination of the question of azimuth.

1. What are the parallaxes of the stars employed?
2. What are the colours of the stars?
1. The determination of the parallaxes of the stars employed in the research, relative to those of stars of 7th or 8th magnitude with a good heliometer, could be accomplished in two years with all desirable accuracy. I would write further on this point but that I have said all that I can say on this subject in the memoir which I mention in the accompanying letter, and which I hope to send you in a few months after I reach England.
2. If there be anything in Forbes's researches (which I am inclined to doubt on the evidence of Newcomb\*), red and blue light have sensibly different velocities, and it would be very interesting to see what evidence Nyrén's investigations give on this point. If the redder stars give a smaller value for the constant of aberration than the whiter or bluer stars, it would be a fact of much importance.

*Royal Observatory, Cape of Good Hope:*  
1883 Dec. 19.

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*Remarques sur les "Notes on Nyrén's Determination of the Constant of Aberration" de M. Gill. Par M. Nyrén.*

Il m'a été de beaucoup d'intérêt de lire les remarques faites par M. Gill sur mon mémoire, "L'aberration des étoiles fixes." Quant aux doutes qu'il exprime sur quelques points du mémoire, je m'empresse de répondre ici en quelques mots.

Par rapport aux mires destinées à contrôler la constance en azimut de notre instrument du premier vertical, il a été dit dans mon mémoire que leur construction a été tout à fait analogue à celle des mires de la lunette méridienne. Les piliers portant les mires sont construits en briques et s'enfoncent de 10 pieds dans le sol, étant isolés du terrain environnant par un intervalle assez large. Les parties supérieures des piliers sont entourées de planches, et l'espace entre les planches et les piliers est rempli d'étoffe de chanvre. Des maisonnettes en bois revêtues de mortier entourent l'ensemble de ces constructions. Autour de ces cabanes il y a des arbres en partie très vieux. Pour la mire d'Est, les arbres n'abritent pas complètement la maisonnette

\* Newcomb said when here, that, with his powerful apparatus, if there had been a difference of  $\frac{1}{1000}$  part in the velocity of red and blue light, his star (or spot of light) whose displacement, due the time of passage of light, he observed, would have been converted into a spectrum that could not have escaped his attention.